HARVARD UNIVERSITY

DEPARTMENT OF ECONOMICS

CAMBRIDGE, MASSACHUSETTS
August 26, 1936

Prof. Ragnar Frisch
University of Norway
Oslo, Norway

Dear Prof. Frisch:

I want to thank you very much for your kind letter and the generous advise you have been good enough to give me. I trust you will understand a young man's desire to have his product published in the form he is able to give to it and as quickly as possible, and hence will not be offended at my having taken the opportunity to have my article published in the Review of Economic Studies and my request to return the copy of the manuscript as soon as you have done with it. But nothing will give me greater pleasure than to enter into a full discussion with you, privately or publicly as you choose, of a subject of which you are the acknowledged master. However I may come out, I cannot fail to derive the greatest benefit from it.

The main point of your objection to my analysis, as I understand it, is that in the general case the real income function is not entirely arbitrary. In the first part of paragraph 2 on the reverse of page one of your letter, you construct your example on an assumption of expenditure proportionality and indicate that in this case for a real income convention, the flexibility is constant with respect to a shift in base. Under this condition, I have proven the flexibility is constant not only in respect to a shift of base, but also in respect to any change of real income. On this point I assume we are now in complete agreement. As to the second part of your argument, in which you drop the assumption of "strict income proportionality," I do agree that for any given convention, the difference between two flexibilities for a change in base path will be finite so long as neither of the flexibilities is infinite; but in this latter general case, the difficulty still remains of choosing one from an infinite number of monotonic functions of utility as the real income measure.

With kind regards,

Sincerely yours,

Abram Bergson

Abram Bergson

September 15th 1936

Dr. Abram Burk,
Department of Economics,
Harvard University,
Cambridge, Mass.,
U. S. A.

Dear Dr. Burk,

Thank you for yours of August 26th. I still think that no harm would have been done in modifying your presentation on certain points, but you are of course the judge whether you want it published in its present form. If you deem it important to have it appear in its present form I think The Review of Economic Studies is as goodaplace for publication as any. Of course there is no question of my being offended for your having taken the opportunity to publish the paper in this review.

Since your paper is accepted by The Review of Economic Studies I take it that you have a second copy of the manuscript which has been presented to the editors of this journal, and that therefore it is no particular hurry in returning to you the copy which is now in my possession. I may want to look certain parts of your mathematics over more carefully. If you want the manuscript returned immediately or if you want me to forward it to The Review of Economic Studies, please write or cable me (as a cable address Professor Frisch, Oslo, is enough).

With kind regards,

Yours very sincerely

Ragnar Frisch

Your proposition that independence and expenditure proportionality entails not only that the flexibility is an indifference function but even a constant, is I think important. Since I have read your paper I have myself proved this by another more direct - and I may add - much simpler method. The significance of this fact for money flexibility measurements depends of course on the set of assumptions that underlie the measurements. So far measurements have been made under the following three different sets of assumptions :

I. Expenditure proportionality, independence, flexibility a constant. (Section 26 in my book "Confluence Analysis", further the work of Dr. Waugh, Econometrica 1935).

II. Expenditure proportionality, not independence, flexibility a constant. (Section 27 in "Confluence Analysis").

III. Expenditure Proportionality, independence, flexibility changing with real income. (My Paris studies and the measurements in "New methods").

In the case of measurements of the type I, your proposition offers a welcome proof that the third assumption underlying this kind of measurement is not an independent assumption but follows from the two first.

On measurements of the type II your proposition has, as far as I can see, no direct bearing. I have not thought the matter through, but it does not seem that the assumptions of expenditure proportionality and flexibility constancy entails non independence.

With regard to measurements of the type III I agree with you that your proposition raises a serious question. But I do not think that your proposition - at least not in the form in which you have so far succeeded in putting it - actually proves that the methods are unsound. When the matter is thought through I think one will find that the assumptions in question come in more or less as principles of "osculating interpolation". Even if the assumptions of expenditure proportionality and independence are made for the set of points in a given vicinity that are used for obtaining one flexibility measurement - these assumptions need not be made with regard to the properties of the choice field at large.

By mentioning only one of the above cases, namely III, I think that some injustice is done against the kind of approach of utility measurement which I have tried developed.

With kind regards,

Yours very sincerely

Ragnar Frisch

19th July 1936.

Mr. Abram Burk,
Department of Economics,
Harvard University,
CAMBRIDGE, MASS.

My dear Dr. Burk,

Thank you for your letter of June 29th with which was enclosed your MS.:- "Real Income Expenditure Proportionality and Frisch' New Methods of Measuring Marginal Utility."

I have read your MS. with great interest and find that it contains several points which ought to appear in our columns. I think, however, that in certain parts the exposition ought to be somewhat modified. Perhaps it would also be advisable to break the paper up into two portions to appear separately.

In the first place, I think it is unnecessary to repeat the formulae and considerations as you have on pages 5 to 12. Practically all of this is already stated in my "Annual Survey", to which you refer. If you use the same notations as I have used, a reference to the various formulae in the "Annual Survey" would suffice, and we should save a whole lot of type-setting. I do not see that anything is gained by the change of notation which you have introduced. Of course one notation may be just as good, or bad, as another, but if a specific analysis is made of a paper which has already appeared, it will no doubt cause less confusion to use the same notation as the one in the paper to which so much reference is made all along the line.

Regarding your thesis of the impossibility of performing a measurement w or of $\dot{w}$, I would like to make a few comments, which may clarify the situation. In the light of this you may wish to modify your line or argument somewhat. I think it would be more effective to have a preliminary exchange of views by correspondence and then present the final result to the readers in a more condensed and digested form. I plan myself to write another paper elaborating somewhat on the technique of measurement, and I may now profit by incorporating in this paper the results of our exchange of views.

Your main thesis is I understand that, in the case where w do not have expenditure proportionality, the notion of real income is entirely arbitrary and that the money flexibility function is, therefore, also entirely arbitrary. You say on page 17:- "While in the case of expenditure proportionality the real income function is determined to the extent of an arbitrary scalar constant, in the general case no restriction on the real income function, in addition to the a priori one that $r'(U)>0$ is justifiable." With this I cannot agree. Already a commonsense consideration of the whole situation should convince one that there cannot be this perfectly discontinuous change between the two cases. Suppose, for instance that there is only a very slight lack of expenditure proportionality. Suppose that the lack of proportionality is so small that it is just barely perceptible over and above the errors of observation. Does it seem reasonable to say that in this case the real income function is entirely arbitrary, while, in the case where the slight amount of income disproportionality had been absent, the real income function would have been entirely determined (apart from the arbitrary scalar constant, which of course is of no avail)? I do not think this is reasonable and an exact mathematical scrutiny of the situation shows that it is actually wrong.

Indeed, suppose that a certain field of price situations t is given. Suppose we select arbitrarily two different base paths, and assume in each case money utility along the base paths to measure real income. These two definitions will lead to exactly the same money flexibility function φ, if there is income proportionality between the two paths chosen. If we do not have strict income proportionality, the two money flexibilities will not be exactly the same, but the difference between them will not surpass a definite number that depends on the maximum expenditure disproportionality that exists along the two paths. In other words we just have a sort of a "Heisenberg indeterminateness relation", and the relation will be all the closer the smaller the disproportionality between the two paths. It would not be difficult to work out the exact formulae of the indeterminate relation in question. In my forthcoming paper I am going to do this. If we let the selection of the base path be arbitrary over the field of paths considered, there will still be a maximum indeterminateness in φ, which can be indicated in terms of the maximum disproportionality that exists in the field.

I am inclined to believe that actual measurements, according to the generalised method, will tend to show that the indeterminateness relation that exists in reality is of the same order of magnitude as the accidental errors we must reckon with. If that is so, the definition of money flexibility is determinate for practical purposes.

There are also some minor points in your paper which I would like to mention.

In your footnote 36 on page 16 you say:- "Frisch believes constancy (expenditure proportionality) in itself leads immediately to the convention (real income = base path money income)". I did not mean to say that. If we have expenditure proportionality it can be proved that we must have $P_t(I) \equiv$ constant, independent of I (if $P_t(I) \equiv$ constant for one t, the same must of course be true for any other t in the case of expenditure proportionality since we have quite generally $P_t(I) = P_s(I)P_{st}(I)$). I only meant to say that in the expenditure proportionality case the most plausible way out is to assume $P_t(I) \equiv$ constant. This identity has of course still the nature of a convention, but it is to my mind the most plausible one, and I think you agree with this.

On page 13 you say that the choice of the real income convention represents two elements of arbitrariness: the choice of the base path and the choice of the real income function along this base path. The arbitrariness which exists can hardly be expressed in this way since the fact that a certain function is chosen along one path is equivalent to choosing a function (perhaps another one) along any other given path. The function along one path follows uniquely from the function along any other path, but of course this is a minor point and largely a matter of terminology. You are of course aware of the fact that in my "Annual Survey" I pointed out this element of arbitrariness (see, for instance, the remark after (7.23): "A similar reduction does not take place with the P terms").

Your discussion of the consequences that follow from the two assumptions, 1) expenditure proportionality and 2) the existence of an independent commodity group are interesting. I wonder whether it would not be a good plan to let this part appear as a separate paper and say in the introduction that a subsequent paper will discuss the extent to which this constituted a restriction of my method of flexibility measurement. I shall be looking forward to hearing from you in this respect. In the meantime I would like to keep your MS. here in order to be able to study the last part a little more closely.

You may be interested to know that, at the meeting of the Econometric Society in Namur, 1935, Marschak presented a note on this same topic, but I do not know whether it was ever published anywhere. You may write to him about it. His address is All Souls College, Oxford, England.

Best regards.

Cordially Yours,